Group Interview 5

VR: Ken, tell us a little about some of the spin-offs that you had from the river continuum and that affected your research for the future.

KC: Well I think one was we started trading leaf packs around the world. And that's how I got to know Ian Campbell He sent us eucalypts and we sent him roadies, and they're both "shoe leather." And so we started looking at the temperature directed rates of disappearance. And it turned out that his bugs and my critters recognized rhodies just as they would eucalypts, and our bunch recognized eucalypts just as they would have roadies. And they all went at the same rate. Temperature corrected rate. So then we traded some packwork with people in Italy. That's when one of Bonnomi's students came over and worked with us in Maryland at that time. And then there's a lot of leaf trading going on now. Leaf pack trading.

42:23

VR: Yes, there's a big group in Plon that are running it.

KC: Yeah, and then one of the other spin-offs was one of my graduate students that did a PhD with me, had done a masters with Jim Barnes at Brigham Young. And so that got Barnes involved in the—in fact, Jim Barnes came to Kellogg and started to do leaf-pack stuff there in lakes. So there was a lot of sort of leaf stuff. And then I think the main spin-off for us was we mounted a big experiment to try to get at the issue of what the best analog was for leaf decomp. And so we had a tributary where there were, I think two Basswood trees in the whole basin of this small tributary, and we got ladders and stripped those trees of all their leaves. And then we introduced the leaves into the stream and tracked what natural leaves did, what leaves in bags, mesh bags is what most everyone was using, and then what leaf packs did. And I thought we showed real conclusively that the brick packs tracked the aerobic natural leaves perfecty. The mesh bags tracked the leaves that got buried perfectly. So if you turn a leaf black, it'll look like the ones in the mesh bags. And they handled, of course, whether they could get into the bags. All that stuff. But then other people started looking at that and the people at Brigham Young actually did a whole series of experiments with cages of different mesh sizes. You know, it was pretty clear that the two things that were driving the overall decomposition of litter were temperature and oxygen in streams. And so if you limited the oxygen in any way, then you excluded the hypomycete fungi And that was Klug's sort of help. And that got started to be looked at a lot of different places. But just like they're still pulling logs out of streams in Northern California, they're still putting leaves in mesh bags. And so I'm not sure we've exported efficiently. But I think a lot of this exploring this sort of thing, and then whether, exactly what the drivers were that let the chemistry part of the thing. That got exported as well. Particularly through a couple of microbial people at Pitt were working on that. And that led to the—you know, if you get the right lipids and put it on filter paper, the bugs will eat the filter paper. So they're really after lipids. And I thought that kind of approach is that, and again I think there's been some exporting of that, but as I said this morning when Charlie Hall called me up, and talked about should we

reinvestigate some of the caloric stuff. And I sad, Well, you know, calories is really all about lipids. That's what the six thousand calories is driven by. How much lipids there are. And so we really need an inventory of lipids, now. Non water-soluble tracers for setting the system up. So I think in the general area of does this recurring system work similarly around the world, it's not the species of the leaf, but there are in fact fast medium and slow ones that are all dependent on cuticle thickness and biochemistry and lipid content, and that sort of thing. And then I think that a number of other labs are certainly, and Suberkrof was with us. And now, and when he left, a lot of that—he fertilized us, and that fertilized the things that he did.

BC: Barnes even brought his cages up into the desert streams.

46:36:

KC: Yeah, right. So there was a lot of that sort of thing going on. So I think that kind of detrital focus and riparian significance was . . . and so, in fact my wife Peggy says that my pictures are boring. Everyplace I go, I pick up litter with some shredders. Always the same hand, this just looks like seeing one leaf—the hand gets older, but it's all the same thing.

VR: What about the functional feeding group concept? That was developed, started a little before the continuum, but it's such a part of the perception that most people have of the continuum. Did that develop much after the studies in the late seventies and early eighties.

47:20

KC: Yeah, again, that was being associated with Klug helped that along a lot, because he was interested in the microbial part of the detrital work, and the experiments that we were doing showed that the fine particle feeding ones were not able to select for quality. Whereas the shredders were definitely able to select for quality. So it was a real sort of strategy shift there once you get to the small particles. You can feed blackfly larvae aluminum particles that are the right size, and they'll eat them happily in a lab, and you can mix sand with detritus in the right sizes and show that the growth is totally controlled by how much sand is in there. There are a lot of things like that, so we were trying to check some of the ideas of the functional group. And that's certainly what I've been working on the last fifteen years, really. And right now we're trying to develop a strategy for basin-wide kind survey of invertebrates. And that's about the time I hooked up with Merritt 'cause he came to Michigan State. In fact, he came in and started doing the aquatic insect stuff that I was doing by commuting from Kellogg up to campus. And so that, the idea that you can continue along the line to get better and better at the "what is it" part of invertebrate work, with just the systematics. But in the meantime, you can do, "What do they do?" parts as they go along with that. And that trend sort of cuts across the "what is it", and Rich and I we'd been working on that together. And one of the ideas of the book was we need to continually track kind of the best ecology information at the generic level for all invertebrates. And that's what all those big tables are about in the

book. But it's never, never quite worked, and for most people it's still what's in the gut contents, and you simply cannot match gut contents with the morphology and behavioral strategies that collect it. So we're still working on that. But now we've—Rich and I, and Peggy's been involved too. We've gone a bunch of different places, in Europe, and in South America and Australia, and tried to see how that system works in different locations. And I think that's the basis for Noel Hyne's statement that you pick up a rock anyplace and you can recognize the bugs. They look the same. There are different families, and maybe different orders. In South America, they're different orders. Or in Australia, there's the stoneflies and the mayfly and so forth. But that's just another signal that the parallel or convergent evolution and morphology of behavior for linking to the resource is pretty universal. So that's really where we've been going. And right now in Northern California, the north coast, we're trying to implant some kind of an invertebrate survey approach where you leave the field with the data. The problem with invertebrates is you leave the field with the bottles, and then it's a thousand years before—And you need to leave the field with the bottles, cause then you can do the rest, if you've got the time, the money, the people. And get at production and other kinds of questions that you might want to ask by sequential samples that you look at in the lab. But you can leave the field with some information. That was the idea. And that tracks real well with people doing fish surveys, cause they leave the field with the data. So now we're trying to couple up the invertebrate and the fish surveys together, cause there's a huge effort to get out the statistical design of how you sample fish populations, juvenile salmonids, and we've just tried to walk right into their design, and say okay, we're going to also look at how the invertebrate communities are set up around these rearing areas for these fish. So that's kind of where, the spin-off from the continuum certainly for us was that. I mean, that's where we really went. And all the people involved helped with that. I mean, Klug for sure, and Bob Peterson in a lot of his work was interested in detritus and partitioning of it, and then with Rich, that's trying to go around the world and look at those systems in other places.

52:11

VR: Thanks very much. Robin, what about at Stroud? What were some of the immediate and maybe a little delayed spin-offs that happened from the continuum work?

RV: Yeah, I think the immediate work was continuing to focus on the benthic communities and their interactions with the input of leaf vegetation; organic material coming in from the water shed. We were looking at a lot of linkages with the floodplains with the hill slopes. We did a lot of manipulation studies on food quality of leaf litter, recognizing the terrific changes in the composition of the eastern deciduous forests with the residual beeches that had low values versus the hardwoods that are more economical to cut and remove for selective use in the change in the composition, and also the fact that we were only dealing with twenty percent forest in a watershed, and eighty percent open land, what was the impact of this kind of configuration on stream communities? We began really early in recognizing that the temperature differences in meadow streams versus woodland streams was very dramatic. I mean, it'd come out of the forest in the summertime, and immediately jump almost immediately ten degrees, goes back into a big

woodlot, cools right back down. So I think in 1973, I began planting trees along stream channels in White Clay I was kind of naïve at it in the beginning. I called up an old friend from University of Maine who worked for the forest service. He was the only one with reforestation on strip mines and had these super poplar trees that would grow very fast, and I wanted shade quick. Quick results. So I called up Mirslov Tropplepovski. He was escaped from Russia, and was a forestry student in Germany and then at University of Maine. And he was enthusiastic. And he couldn't believe the productivity of White Clay Creek soil, floodplain soil versus spoil banks of mining. So we got the trees growing, and shade there, and I think we wanted to look at how efficient insects were in processing various species of leaf litter, in these different temperature zones. And there we came upon the fact that temperature was a major factor in growth of insects, and their fecundity, and those studies went on to be very successful and rewarding. And in another large geographic study. So we were coupling sort of reforestation, what were the best ways to do it, what species of trees would benefit communities, maybe naively. You can never reconstruct a forest to the original, natural state. But it was very important to develop a canopy over these streams. But then again, can you overdo it, because beavers were always in these watersheds, and then essentially got back into White Clay, and I can remember studies as an undergraduate, looking at beaver meadows, and aging shrubs and trees on meadows. So meadows were always a component of streams in the eastern deciduous forest. So maybe it's a mistake to completely reforest the stream, if you don't have the beaver component to make these kinds of shifts and develop meadows. So that's an interesting aside anyhow, but I think my work led more back to the benthic communities, and the nutrition, production, how food was a major component, of where it came from, from the detritus, from outside production. So we still had a very integrative team to look at these questions. And I was also very interested in how water got into streams. And this is particularly important I think in these forested streams in the east because all the uplands are well developed into agriculture, either pasture or farmland, and the runoff from those areas are very severe in the non-crop season. And water gets shunted into these woods. You get terrific erosion. The forest floor isn't well-protected. And now maybe with this earthworm work, we know we have less protection, it's going to be more severe. At that point, we brought Denis Newbold in. He became available, and I had money, and the position came. So we could establish Denis at the lab. So I think that was really a major addition to the capabilities.

58:17

WM: Yeah because in part, Dennis brought the spiral home and the capabilities he had.

VR: And he was a Berkeley product.

WM: He'd looked in logging impacts as part of his doctoral work.

BC: Obviously Dennis has had a lot of impact because I'd like to throw in just a little bit more about—we mentioned earlier about the FPOM transport. I can remember Wayne and I starting out on this project and we're over at his house for a week or so to write that first proposal. We were writing, showing these little simple curves, and we'd put on

these radio tag particles here, they'll go here, and then we'll know a lot. And we started out pretty simplistically, but boy by bringing on Dennis and Steve Thomas, and their modeling capabilities. It just took us—I hate to use this term—to a whole new level, cause I never figured out how many levels there are you can get to. But anyway, these guys really took what we had originally decided to try to do to a level that frankly, I don't know if we'd made it without.

59:28

WM: Yeah, I honestly, it was a great coup that we acquired Denis, because we acquired the modeling mathematical capability, and we were able to marry the spiraling concept with the river continuum concepts and move both very forward. And so it was really great in that regard. So there's a tie with Denis. Not only the spiraling ideas, but then with the Stroud lab. And we've been continuing our collaboration with the Stroud lab on that route and through Jim Brock's connections with the metabolism aspects up until the present time.

60:05

RV: I think it's interesting how the loop from the spiraling concept goes all the way back to Bob Ball and Frank Hooper who originally designed in the late fifties, the introduction of P-32 into Titchin river stream. That was a terrific data set that really never made it into publication, and I guess the data set Bob Ball took it down and gave it to Denis at Oak Ridge, I believe when he retired or went to the Water Institute, when Ball went to the Water Institute on campus.

BC: But that transfer, one of the major interesting things about that whole thing that was almost an aside was that they found the P-32 upstream. And that was the first time we started looking at a little bit of migration upstream. Everybody thought put it in here, it's all going down here. Instead they found it in bugs upstream.

61:13

WM; Well they found it in the treetops, too, so I mean it was a real linkage with the riparian and terrestrial that hadn't been appreciated fully. In fact, I think maybe they didn't appreciate it. They were afraid of the contamination issue rather than that—

JS: They never did that experiment again.

VR: Well we related if we remember the Doe Run spikes that Krumholz would do when it was right above the Heaven Hill Bourbon intake, which I've never drank Heaven Hill Bourbon since.

WM: Well, that's what made it heavenly.

61:53

VR: Robin, any . . .

RV: Well I think that's about all of the major things that I've done afterwards.

VR: Jim

JS: Well, my tack was very different. These guys are hard-core scientists. And my interests turned to bigger rivers, and the issue then, to me was, if all the little streams are dominated by forest—cause that's the reference I was using, a forested system, or some sort of vegetation system, the assumption was that the big rivers were less influenced by that. And when you started to go through the archival records, there was some incredible interaction from all these tributaries, all these upstreams, that while there wasn't necessarily an energy driver in the big rivers from that, but there was a habitat, an artifact, and a floodplain that was very much dominated by both upstream and adjacent vegetation on the large wood. And so I started to go back to the old Army Corp of Engineer Records and pull out, snag records, and again it was aimed where they said it's only happened in the northwest where we had great big trees and it was obvious. It was obvious whether you looked any place in the country. Or even in Europe. And then that got into looking at, well how have those rivers changed so that our perception of the world just doesn't match what it might have involved in the Wilamette paper, and then moved on to something else. I think at that time, then St. Helen's blew. And I was interested in beavers also. I think Amy Ward and Cliff Dahm were very influential. When St Helen's blew, those lakes they said they're all dead. Those lakes were more alive, and the streams were more alive than they'd ever been. It was just billions of anaerobic bacteria. And so I started to look at that. And we were starting to look at these stair steps through log jams and through beaver ponds. And then the chemical changes and the release of N and P and Cliff went on and did a lot of that in New Mexico. It was using those same kinds of techniques. Of looking how essentially a forest or a vegetative kind of step then created whole different nutrient pathways. And indeed we found our fish were bigger there, and more action was going on just below those. But I never really got back. I got more then into taking the vegetation control and applying that. So my career path took an archival bent and then also started to look more and more at how do you translate these concepts or what we know about influence of forests on streams into how we might want to manage them if in fact we wanted to maintain the salmon and other things. But again, you're drawing on this huge group of people. Ken came to Oregon State, and then people that weren't associated with it, like Bilby and Bisson become important colleagues that push forward a lot of thinking about that. Stan Gregory is now into big rivers, in the Hilamette and Wilamette Outlet. So I think that there was an evolution where we grew up in very little streams and then we started to look at it a little differently. And right after, cause I pulled away from the continuum for personal reasons and family reasons in seventy-seven, and went to work for Weyerhauser, and at the time Dick Waring at Oregon State and Jerry Franklin recommended I get on a National Science Foundation panel. So I became a part of the ecosystems panel, the ecology panel, just when they were splitting it between ecology and ecosystems. And intellectually, that kept me alive, and that allowed me to advocate

for things like Jeff Ritchie's Amazon proposal that got started, where again you're looking at a size and scale that we never even thought about when we were out on the Salmon or even looking at the Wilamette next to Oregon State. So in many ways those have come home to roost now twenty years later, with all the stuff that Ritchie's group, and Milac and others have done. And so I feel like I had a part in moving that around. But I think the big deal is still the influence of forest on rivers.

67:05

KC: And a lot of us would attribute you for the historical perspective. I mean, we looked to you. It might have been focused on wood, which was a logical thing

VR: And that's so that you could get some data. You liked data.

KC: We brought you into the Kissimmee basin, and it's quite clear that there was significant cypress in the Kissimmee basin. And cypress, of course, is no part of the restoration, whatsoever.

JS: Right, there are live oaks on the levy.

KC: Yeah, right. So there was a definite big wood story there. In fact, it probably set up the braids to begin with in the river. So that kind of historical perspective which you can tease out of the history of the logs actually was really pretty important, I think.

WM: Our whole conceptualization of how those large rivers looked is influenced by the fact that we thought here and now is what they looked like before. And Jim's given us the insight that they could have been looking very much different.

KC: And they're not planting any cypress trees as a part of the restoration and that's stupid.

BC: Did you leave, or did we throw you out

WM: Well one of the things I just sort of added, is one of the things that you're seeing here is this new opportunities that emerged for me and some sort of life changes that occurred. So there really wasn't the coalescence for going on for additional funding to continue the river continuum. I think that's really important. Bert will probably talk a little bit later about his favorite part, the Middle Fork. But in part, our work on the Middle Fork put us in a position that in 1979, in this same era, there was a major fire on the Middle Fork. And that changed the direction of my research. Using a lot of the tools from the River Continuum, but applying them to long-term effects of fire, initially in the Mortar Creek section on the Middle Fork of the Salmon, later in the Big Creek section, which we sorta had just gone by and were awed at when we passed by it, and incidentally, the Big Creek section is really what I think formulated our ideas about the impact of tributaries and got us thinking and questioning, and developing some thoughts along those lines. And that's reemerged recently now in a whole body of theory that's

taken precedence in the literature. But it really had its original ideas about influence on continuum ideas back then. Anyway, Big Creek was another place where we had fires. Simultaneously with that, Yellowstone erupted and we were invited to come in and do the research on that, and that's extended into a real long-term data set now.

69:55

VR: Let's talk about the Middle Fork studies, and the float trip That was done in the area. Burt, could you put it into time context, too

BC: Yeah, this was in the summer of seventy-eight, and I guess one of the reasons that I like to talk about this is because to me this was still one of the best trips for just stimulation and exchange. I had a ball on this trip. Despite Ken and I getting thrown out of the raft, sunburnt feet, and the things that went with it, it was just a great experience at looking at all these aspects with all these guys, and I just think it was a tremendous trip.

70:33

RV: There was a lot of hard work, and we learned an awful lot about what's on the bottom of the river, and we did a lot of sampling.

WM: We came up with a variety of things about rugosity on the rocks, you know, whether they were smooth or rough, microhabitats, kind of distributional things, diversity responses. So there was a lot of invertebrate sampling, a lot of on-stream processing. Just a great effort in that regard.

BC: We saw a lot of those things we looked for in the continuum site. And we also did analogs. Of course, we couldn't carry the chambers. So I did periphyton and chlorophyll analysis as sort of a surrogate to those in all the different places we stopped. I just think it was one great trip and—

71:24

VR: How long did it last?

BC: Ten days. I remember.

VR: And the five of you went?

BC: Uh, you didn't go . . . Bruce Wallace came with us as a guest. And we started at what Dagger Falls? Went to Indian Creek, sampled in Indian Creek and above and below, same with Loon Creek and Big Creek.

71:44

WM: We were looking at tributary effects.

BC: Yeah, we were looking at what it would be like above where the tributary came in, below, how far the effects go. It was just a real stimulating trip.

KC: We learned how to fly fish when there's stable flies around. I got into those things. And that's a biting insect that's usually associated with agriculture. And this is a pristine river, was full of stomoxies, which breed in detritus. Nasty flies.

BC: To me, it was one of the real highlights. It really was.

WM: And besides us, there were a number of other folks that had been on the technical crews or the research crews or the graduate students. So it was a mixture.

BC: Plus we had a couple of professional guys to do all the cooking and all this, to take care of the amenities so we could work. And it just was a great trip.

72:53

KC: And we got the garbage collectors to collect samples for us. Later on . . .Forest guys would pick up samples for us, cause they were making this run repeatedly.

WM: As an aside, we were caught in a rainstorm at Loon Creek, and it was my first experience of somebody getting near hypothermia, because Robin and Dave Funk I think were sampling in Loon Creek in this downpour and they just kept on sampling. They would not come back. And when they came back, they were somewhat incoherent and silly. You know. But it was recognized, we sort of pulled out, we got a fire going, got warm things into everybody, but a real scary thing on our . . .

BC: To be on, you go out, sample at your place, you come back to the lab. Here you sample, you learn about how to preserve them, at Loon Creek. A plane flies in, brings supplies, flies samples out. This is a whole new game. It was really an experience: Until that last day. We could do without that last day, though, couldn't we?

VR: One of the things I'd like to ask you to all talk about is twenty-five years later, after the 1980 paper came out, there's still some misinformation or misconceptions about what the continuum said. Actually, didn't say, but has been presumed to say. Could we talk about that just for a few minutes and just set the record straight that these things are really still misinterpreted about what the RCC proposes?

74:47

MF: Just a second, let me just make a quick change of tape.

VR: Is that a time because you don't want to talk about this?

WM: No. Because we're out of tape.

KC: I'm not sure I have anything more to add.

RV: Yeah, I'm not sure where we're headed on this.

WM: Well yeah, I thought we covered it. I'm not sure that more and more ground could be gone through. I think the biggest problem was the zonation. The assumption that we had said, by god it's absolute, you know, we'd really intended this sliding scale.

VR: Okay, if you could just do that and then we'll run with that.

JS: Yeah. To me the amazing thing is how literal the science community is.

VR: That's fine. Say that.